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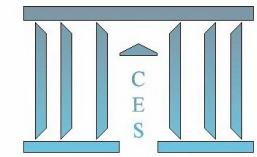

Proving the existence of macropsychological phenomena? The Katona-Tobin controversy over the predictive value of attitudinal data

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2015.11

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http://centredeconomiesorbonne.univ-paris1.fr/
ISSN : 1955-611X

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4th December, 2014

Abstract :

This paper analyzes the controversy between George Katona and James Tobin that happened at the end of the nineteen fifties. The central problem of the paper concerns the nature of psychological phenomena. While Tobin defends that economic behavior stems from the individual, Katona tries to develop a macropsychological approach in which the individual plays a secondary role. The controversy thus reveals the arguments that initiated the developments of microeconomic theories and the construction of microeconomic data during the nineteen fifties.

Keywords: Macroeconomics, History of behavioral economics, Attitudinal data, Economic prediction

JEL classification: B23, C83, E03

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Introduction:

"Psychology is overwhelmingly micro because it is only the individual who reacts, learns, thinks, and has emotions. Social psychology also analyzes mostly how individual behavior is influenced by belonging to groups, although group norms and social forces are occasionally discussed. Does macropsychology therefore belong in sociology, or is it something hazy and unnecessary ?" (Katona, 1979, pp. 120-1)

"How can economic psychology proceed? What are its tools? Like psychology, the study of behavior, economic psychology, the study of economic behavior, must be an empirical discipline. Theory has an important function in empirical science because fact finding must be based on hypotheses in order to lead to valid generalizations and ultimately yield verified or improved theories. But the tools of econonomic psychology are empirical." (Katona, 1947, p.54)

To be recognized by the scientific community, a discipline must be able to provide evidences that support its theoretical assertions. Explaining the observed phenomena can be seen as a first step. Predicting phenomena not yet observed is a second step that provides a more convincing test of the relevance and potential uses of a theory.

This article focuses on one of the first controversies between a psychologist, George Katona, and an economist, James Tobin. Although it was briefly mentioned by Edwards (2012), this controversy has neither been studied in the literature centered on George Katona (Hosseini, 2011; José Edwards, 2010, 2011), nor in the recent empirical literature devoted to evaluate the relevance of attitudinal[1] data in forecasting macroeconomic aggregates. This controversy is related to the following question: By which means can we exhibit a causal relationship between individual attitudes and aggregate behaviors such as aggregate consumption or aggregate savings? Using the same body of data, both actors develop and try to support different views. Two positions emerge. While for Tobin, aggregate phenomena should be reduced to the amount of individual behaviors, Katona argues that they should be seen as social or macropsychological phenomena, which have no equivalent at the individual

[1] For Katona, attitudes are "generalized viewpoints with some affective connotation, which influence perceptions and cognitions and, above all, behavior" (Katona, 1958, p. 60). Attitudes include: expectations, motives, aspirations, goals, values, frames of reference (Curtin, 1983, p. 509). However, Katona states that "... the terms used and their exact meaning is not very important" (Katona, 1951, p.36). This concept is central to the corpus of social psychology, a predominant discipline during and after the Second World War in the United States. It has received many definitions in the history. The article does not look closely at the definition of attitudes, firstly because Tobin and Katona do not fundamentally disagree on it, and secondly because the controversy is centered on data obtained throught surveys, regardless of what these data represent theoretically, *i.e.* attitudes. Attitudes are essentially a concept related to the individual. However, for Katona, individual attitudes are influenced by society, or in other words by the attitudes of other members of society. The nature of these attitudinal data is detailed below.

level. Studying this controversy allows not only to explain why Tobin (and after him many economists such as Adams, Juster and Okun) rejects Katona's program, but also to shed light on some issues surrounding the debate over microfoundation of macroeconomics. Before addressing those questions, we begin with contextual elements.

George Katona is one of the first psychologists to develop a psychological approach to economic problems (Hosseini, 2011; Sent, 2004). Unlike Herbert Simon, Katona focuses on macroeconomic issues, although he also provides many contributions to microeconomic theory including the behavior of firms (Katona, 1945, 1946, 1955) and the question of rationality (Katona, 1953)[2]. Katona's behavioural economics ("old behavioral economics" according to Sent, 2004), combining social psychology and *Gestalt*, has little in common with behavioral economics today ("the new behavioral economics" according to Sent, 2004). However, this controversy is very interesting because it provides an early example of the confrontation between psychology and economics, their assumptions, the empirical data used, and their explanations of economic phenomena.

The encounter between social psychology and economics took place following the development of social psychology in the context of World War II and after the end of the war. During the war, most of the prominent social psychologists (Rensis Likert, Theodore Newcomb, Angus Campbell) grouped together in institutions financed by the Federal Reserve to analyze and guide policy reforms related to the context of war. One of these institutions is the *Division of Surveys* of the *US Department of Agriculture*, led by Rensis Likert[3] from 1939 to 1946, whose role was to study the reactions of farmers to agricultural reforms during the Second World War (Skott, 1943). The *Federal Reserve* interest for social psychologists is mainly due to their research methodology which, based on attitudinal datas, reports subjects reactions towards reforms (for example farmers in the *Department of Agriculture*). These surveys provide helpful studies on the causes behind the reluctance of farmers, and therefore enable to develop solutions in order to meet their expectations (Skott, 1943) [4]. In 1945, following this partnership, the *Federal Reserve* asks the *Division of Surveys* to conduct a study providing information on households liquid asset holdings in order to "appraise the Federal Government 's bond selling campaign"(Smithies et al., p.5). This cooperation proved "distinctly useful" since the surveys "led to significant changes in the Treasury's bond selling

[2] See Hosseini (2011) for more details on the scope of Katona's work.
[3] Assisted by Newcomb, Campbell, Leslie Kish and Daniel Katz.
[4] Attitude Surveys are strongly linked to behavioral models built around the concept of attitude, and are the main tool for data collection since the work of Louis Leon Thurstone (1927, 1928).

techniques" (*ibid.*). After the war, the US government kept interested in asset holdings since they cared about the problems of post war inflation and the "effect on consumption of the large accumulation of cash and Government bonds"(*ibid.*).The *Federal Reserve* thus asked the *Division of Surveys* to conduct a *National Survey of Liquid Assets* (NSLA). NSLA will be renamed *Survey of Consumer Finances* (SCF) in 1946. In the meantime (in 1946), the *Division of Surveys* is dissolved and its members (Katona, Likert and Campbell)[5] grouped to create the *Survey Research Center* (SRC), a research laboratory based of the University of Michigan.

Katona joined Likert in 1945. Having previously conducted survey studies on behalf of the Cowles Commission (*Price Control and Business*, 1945[6]) from January 1943[7] until late 1944, Katona naturally came in contact with Likert (Katona, 1972, p.14). Although Katona is not an economist[8], his great theoretical and practical knowledge of the field makes him particularly suitable for the direction of the NSLA and SCF. Taking the direction of these surveys, Katona becomes an interface between social psychology and economics. Indeed, the SCF purpose is mainly to collect data on economic variables: accounting data on the ownership of liquid assets (cash savings, bonds, stocks), data on the purchase of durable commodities (automobiles, household appliances), and other socio-economic data (household size, income, number of children, etc.). In addition, questions about attitudinal data are asked (these will be described in Part I). The purpose of the survey is to study the determinants of savings and consumption of households (mainly consumer durable goods). Those determinants are widely debated by economists in the postwar period, and a set of criticisms on the Keynesian demand function emerges (Smithies, 1945 Mosak 1945; Woytinsky, 1946)[9].

This database will provide a new field of investigation for economists[10] who lack the data to

[5] The establishment of the SRC is an opportunity to enlarge the circle of social psychologists. After the death of Kurt Lewin in 1947, Dorwin Cartwright, Ronald Lippitt, Leon Festinger, John RP French, and Alvin Zander will join Likert in the SRC.

[6] For a summary of the content of this book see Edwards, 2012.

[7] Cowles Commission, *Report for period* 1942, The University of Chicago, Cowles Commission for Research in Economics, 1943, consulted on http://cowles.econ.yale.edu/P/reports/1942.htm

[8] Indeed, Katona never got a PhD degree in economics. However, his interest in economics goes back to the 20's. After his PhD in experimental psychology in 1921, Katona integrated a research department of a German bank in Frankfurt and wrote an article on hyper inflation, arguing that hyperinflation is a phenomenon of mass hysteria. From 1926 to 1933, he became assistant editor of The German Economist (Katona, 1972 p.12). Gustav Stolper, the journal editor became his mentor in economics. He is also the German correspondent for the *Wall Street Journal* and *Dow Jones Ticker* from 1930 to 1933. He immigrated to the United States in 1933 and worked as a consultant for European investors. He joined the *New School for Social Research* in 1939, where he teaches a course on the war economy. This course will give rise to the publication in 1942 of his first book combining economics and psychology, *War Without Inflation* (1942).

[9] See Edwards, 2012, p.13 for details.

[10] Tobin says in 1972 : "Katona was the great entrepreneur of survey data collection, and for this alone the economics profession owes him an immense debt. In the early postwar years economists were still convinced that rigorous sophisticated methods could make time series of economic aggregates disclose simple reliable macro-relations. [...] But as primitive Keynesian functions failed and competing hypotheses of greater complexity were advanced to fill the vacuum, the importance of household survey data came to be appreciated. Meanwhile Katona and his colleagues [...] were busy providing

test, develop and estimate models in the context of the emerging econometrics. Lawrence Klein, in particular, integrates the University of Michigan for two years (from 1950 to 1953) in order to analyze the possible uses of the SCF data, and try to identify ways to use them to develop micro funded macroeconometric models (including the model of Klein Goldberger) who were constructed in this university[11].

This is also the case for James Tobin, who will do a research stay in 1953 for the same reasons (Tobin, 1957, p.3). Katona will thus work in close contact with Klein and its econometric program, for both the research and the teaching part[12]. They will write a paper together in 1951[13]. During this period, Katona builds a friendship with Tobin, the latter still recalling in 1972 his admiration for Katona's work (Tobin, 1972, pp. 37-9). Katona therefore has a central role in the development of demand theory. First, as director of the SCF, he provided the first microeonomic data to study demand issues and, second, he was actively involved in theoretical debates on the relevance of psychological variables, and their propensity to explain saving and consumption behaviors. These theoretical debates are closely linked to US political issues as defined by the 1946 Employment Act: maintaining a high employment rate and a low inflation rate.

To this end, the *Federal Reserve* needs tools to predict future changes in economic conditions, and identify possible ways for economic policy (to prevent massive asset sales, to stimulate consumption, etc.). Thus, regarding the production of statistics, "the Federal Reserve System might well expand its statistical collection and analysis programs *where it has special interest and competence*" (Talle letter (Chairman of the Subcommittee on Economic Statistics) to Martin (Chairman of the Board of Governors), Smithies *et al.*, December 14, 1954, my emphasis). In 1954, the Federal Reserve gathered five task groups to evaluate the adequacy of statistical programs[14]. The conclusions of the reports issued by these committees will have important implications for statistical institutions in terms of funding, but also in term of scientific orientation. The *Committee on Consumer Expectations* (called *Smithies Committee*

[...] an invaluable data base. Moreover, [they] pioneered in reinterview surveys: The profession's appetite for panel data of this type is now almost insatiable." (Tobin, 1972, pp. 37-8)

[11] Klein's articles over the periods are: Klein L., *Estimating Patterns of Saving Behavior from Sample Survey Data*, Econometrica, Vol. 19, No. 4, 1951; Klein L., *Assets, Debts, and Economic Behavior*, in Studies in Income and Wealth (Vol. 14), NBER, 1951; Klein , L. R. et Lansing B., *Decisions to Purchase Durable Goods*, Journal of Marketing, Octobre 1955.

[12] See : University of Michigan, *Survey of the behavioral sciences : report of the faculty committee and report of the visiting committe,* Ann Arbor, University of Michigan, 1954, pp. 151-3.

[13] Katona G. et Klein L., *Psychological Data in Business Research*, American Journal of Economics and Sociology, Vol. 12, No. 1, The Psychological Approach to the Social Sciences (Oct., 1952), pp. 11-22.

[14] The five committees are the following: *Savings Committee on Statistics*, *Committee on Inventory Statistics*, *Committee on General Business Expectations*, *Committee on Plant and Equipment Expenditure Expectations*, and the *Committee on Consumer Expectations*.

in this paper), chaired by Arthur Smithies, composed of seven experts including James Tobin, will produce a critical report on the SCF. These criticisms, Katona's response and the following controversy between Katona and Tobin between 1958 and 1959 are the subject of this paper.

The central issue of the controversy between Tobin and Katona is: Are attitudinal data collected at the individual level valuable to predict aggregate consumption? On a common database (the corpus of SCF), Tobin (following the conclusions of the Smithies Report) and Katona will have different ways of understanding the problem, and so different answers. The former understands psychological phenomena as an individual phenomena. Therefore he bases his answer on individual models and empirical tests. For Katona, the psychological phenomena measured by the SCF are aggregate phenomena or, more precisely, macro psychological ones. Thus for him, tests conducted at the individual level are not conclusive.

In the first part, after having explained the contents of the SCF, the article analyze the findings highlighted by the *Smithies Committee*. The report give birth to a composition paradox[15]. In the second part, the paper details Tobin's position and the argument behind it. Finally in the third part, the article focuses on the position of Katona.

I) The Smithies Report: the foundations of the controversy

George Katona has originally little freedom in the design of the surveys, the specifications being set by the *Federal Reserve*. Thus, the major part of the SCF focuses on the inventory of financial assets at the time of the survey, an inventory of sales or purchases of assets during the year preceding the survey, and a set of questions regarding future uses of the assets[16]. Another set of questions is designed to establish an inventory of durable goods, the number of cars, radios, appliances available to the household. Questions are also asked about the intentions to buy cars and other durables in the coming year[17].

[15] *Composition paradox* is used here rather than *fallacy of composition*. Paradox means that there is an observed contradiction between an aggregate phenomenon and the same phenomenon at the individual level. The term fallacy means that there is an inference made from a law observed at the individual level to the aggregate level. In the case of the attitudinal data, the contradiction between aggregate and individual scales is observed. See Ernest Nagel, *On the Statement "The Whole is more than the sum of its parts"*, in Paul Lazarsfeld and Morris Rosenberg (eds.), The Language of Social Research, 1955.

[16] The explanation is based on the study of the 1947 and 1955 surveys questionnaires which are available on the website : http://www.icpsr.umich.edu/icpsrweb/ICPSR/.

[17] These questions are : "1) Now we'd like to get an idea of how much money you personnally are planning to put into large items that people haven't been able to get during the war. Let's take cars, for example. Do you expect to buy a car in 1946 ? How much do you expect to pay for it ? Will it be one of the new models ? 2) How about large items, like furniture,

In order to conduct its own research, Katona manages to implement in 1946 a set of questions designed to collect the opinions of households, their attitude, toward past and future macroeconomic evolutions, and toward their own financial situation[18]. Those questions are: "1) Would you say you people are better off or worse off financially now than you were a year ago? 2) Do you think that a year from now you will be making more money or less money than you are now, or will you be making about the same? 3) Now considering the country as a whole, do you think we will have good times or bad times or what during the next year or so? 4) What do you think will happen to the prices of the things you buy during the next year - do you think they will go up or down or stay about like they are now?". Whereas items 1) and 2) are *attitudes* related and reports households' feeling of their own situation, in items 3) and 4), subjects give their *expectations* about the whole economy[19]. Designed to understand psychological aspects of economic behavior in the spirit of social psychology tradition, those questiones are usually supplemented by open-ended questions - "Why do you say so?" -, that leave room for the respondent to express and detail their opinions. On the basis of initial attitudinal data tabulations, Katona will send reports to the *Federal Reserve* on the state of the psychology of households to provide supplements to other forecasts[20]. These reports will improve other forecasts, leading the *Federal Reserve* to increase funding of the *Economic Behavior Program*, notably for the construction of *Interims Surveys*[21].

Katona's goal, to collect psychological data in order to build a psychological theory of economic behavior, seems therefore to meet the *Federal Reserve*'s objectives: to establish an accounting diagnosis and provide forecasting tools. However, many disagreements between the government officials and Katona emerge (Smithies *et al.*, 1955, p. 68-9), particularly on the most effective way (in terms of cost efficiency) to reach the objectives[22]. Financially

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refrigerators, radios, household appliances and so on ? Do you plan to buy anything of that nature during the forthcoming year ? How much will you spend on these things ? 3) Have you thought of spending any money for building or buying a house in the next year or so ? How much do you plan to spend ? How do you think you will finance this ?" (McNeil, 1974, p.1).

[18] Katona argued to the *Federal Reserve* that these questions were needed to put in good conditions the respondents, and thus to obtain more accurate data (Likert, 1975). This argument is present in the report of the *Smithies Comittee*: "Indeed these questions are valuable if only because they greatly help in establishing "rapport "between interviewer and respondent" (Smithies *et al*, 1955, p.2.) We find the argument also in Tobin's paper (Tobin, 1959 p.11).

[19] For Katona, expectations are a subgroup of attitudes, "those attitudes that represent the extension of the time perspective into the future and shape current behavior"(Katona, 1958, p. 60).

[20]These reports will be regularly published from 1949 in the *Federal Reserve Bulletin*. The report of the *Smithies Committee* makes a detailed account of these reports : See Smithies *et al*, 1955, pp.. 38-45.

[21] Interims Surveys are ponctual surveys, conducted in parallel with the SCF and focused on the collection of attitudinal data. Through these, Katona asks a great number of open-ended questions about households perceptions on the government actions, the impact of wars on the US economy, etc.

[22] "While $150,000 is a very small sum of money in comparison with the aggregate outlay of various government agencies for economic and financial statistics, it is a relatively large amount to pay for a survey involving only 3,000 respondents. We

speaking, the SCF is much bigger than the previous surveys conducted by the *Division of Surveys*, and the question of efficiency arises especially since the *Federal Reserve* intends to conduct the SCF for an indefinite period. The *Federal Reserve* therefore arranges a task group to determine the efficiency and relevance of the SCF, focusing mainly on the issue of the usefulness of attitudinal data (those in the SCF and those obtained through *Interim Surveys*) to predict economic aggregates. This committee includes a large majority of economists (Arthur Smithies, James Tobin, Hazel Kirk, Guy H. Orcutt, Harold C. Place, Bert Seidman), one psychologist (Vernon G. Lippitt) and one sociologist (Samuel S. Stouffer). While, so far, Katona had met economists like Klein and Tobin in the context of research seminars, in 1955 he meet them in the context of an evaluation committee:

> "[We] attempt to appraise the *usefulness* of the Survey of Consumer Finances. We consider particularly the *accuracy* of the Survey statistics and the contribution that attitudinal data have made to *economic prediction*." (Smithies *et al.*, p.9, my emphasis)

The Committee is thus clear on the scope of the SCF, and the intended use of the attitudinal data: precision and prediction[23]. The heart of the debate, that will develop later, is not centered on the utility *in fine* of the SCF, Katona praising the potential of its program for forecasting[24]. The debate will concentrate on the arguments and statistical evidence given by the *Smithies Committee* to establish its criticisms and recommendations.

The main conclusions drawn by the committee are the following:

(i) "Intentions and attitudes seem to have been useful in predicting the general strength of consumer demand...". (*ibid.*, p.2).

(ii) "Survey of Consumer Finances reinterview data suggests that buying intentions are useful but by no means perfect predictors of the subsequent buying behavior of individual respondents" (*ibid.*, p.2).

(iii) "It has not yet been proved that expectations and attitudes, *other than buying intentions*, add to the predictive value of survey data" (*ibid.*, p.2, my emphasis) This criticism is nuanced in the *Smithies Committee* report, but will be much stronger in Tobin's words[25].

These findings have important implications for Katona, since it leads to question the data

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are duty bound to look carefully at this expenditure, with an eye to possibilities of achieving greater efficiency or economy." (Smithies *et al.*, 1957, p. 66)

[23] "The Federal Government has found that in order to carry out its responsabilities under the Employment Act of 1946, estimates of consumer's expenditures for the forthcoming year is indispensable" (Smithies, p.12).

[24] "It is time to emphasize once more that prediction, at least in the fairly direct sense in which we have been discussing it, is by no means the only use of attitudinal data, *although it is the use which the survey Research Center itself has almost exclusively emphasized*." (Smithies, p.66, je souligne)

[25] "Buying intentions have predictive value; other attitudinal questions do not." (Tobin, 1959, p.10)

collected, and thus to threaten his research program:

> "Interim surveys, which collect solely attitudinal data, require more substantial justification" (Smithies, p.66).

Items (i) and (ii) show that the committee makes a distinction between tests at the individual and aggregate scale. Tests at the individual scale are performed through reinterviews. Households are surveyed once at time t and then again at time t + 1[26]. By comparing the attitude of a household measured at time *t*, and the behavior of the same household at time *t + 1*, we can study the rate of achievement of intentions to buy (or the effect of attitudes on behavior). Testing at the aggregate level involves comparing a measure of an aggregate of attitudes calculated with the entire sample of individuals surveyed[27], with an average measure of expenditure (*e.g.* percentage of disposable income spent for durable goods, cars, appliances, the savings rate, etc.) [28]. While emphasizing the predictive value of attitudes at the aggregate level, the *Smithies Committee* insists on the need to provide tests at the individual level. Those individual tests are in fact those who justify item (ii) and (iii), that is to say that buying intentions data have better predictive value than other expectations and attitudes. The article now studies the reasons highlighted by the Committee to justify this position.

The report first insists on the fact that the value of the SCF compared to previously available data (before 1955), lies in its microeconomic perspective:

> "Providing aggregate data is neither the sole nor the main purpose of the consumer Surveys. Their main purpose is to provide data regarding the relationship among the numerous variables relating to consumer behavior." (Smithies *et al*., 1955, p. 36).

Prior to 1955, forecast models were constructed from time series of aggregate data. Two general problems that arise from time series data are highlighted in the report: (i) indirect statistics obtained from aggregates do not provide "the type of information needed to explain consumers' behavior." (Smithies *et al.*, 1955, p.13). (ii) "Time series data are usually available for a number of years that is too limited to provide *adequate tests* of the various hypotheses that can plausibly be made" (*ibid.*, my emphasis) [29]. Thus, "with respect to the testing of

[26] In 1955, three reinterviews waves are available. A first wave of reinterviews was conducted in early 1949 on a sample initially surveyed in 1948, a second one at the end of 1949 on a sample initially surveyed in 1949 and finally the third one in 1953 based on a sample initially interviewed in 1952. The sample reinterviews are usually conducted six to nine months after the first interview. The *Smithies Committee* conducted its tests on the third wave of 1953, given that it is the one with the most observations. See Smithies *et al.*, 1955, pp. 61-66.

[27] Several calculations are performed. When it comes to make a test of buying intentions at the aggregate level, the committee uses the percentage of spending units who intended to buy on the total sample. To establish an aggregate measure of attitudes (*e.g.* expectations of an improvement of business conditions for the economy as a whole), the committee uses the ratio of favorable to unfavorable + favorable responses.

[28] The data used for aggregate consumption time-series are not necessarily taken the SCF. The committee also uses data from the *Department of Commerce*, and the data from the *Securities and Exchange Commission*.

[29] The report outlines other difficulties arising from the use of time series: (i) It may be that the time series observations are dependent on each other (autocorrelated in a modern terminology), (ii) time series of economic aggregates generally fluctuate

hypotheses and the derivation of prediction formulas, the use of survey data can reduce some of the difficulties of the time series" (*ibid.*, p.16). SCF data are interesting because they can produce cross-section data, and thus offer thousands of observations. Although the *Smithies committee* seems sensitive to the theoretical differences between individual hypothesis and aggregated hypothesis, the report clearly shows that in one case (time series) the empirical evidence is always poor, but in the other (cross-section) they are more reliable[30].

For example, at the individual level, "it is often possible to find a great deal of independent variation between the various possible explanatory variables. This makes possible investigation of their individual importance"*(ibid.*, p. 17*)*.

Ultimately : "Adequate survey data, while not dispensing with the need for agregative time series, can greatly reduce dependence on them *for testing hypothetical relations*. However, since it is the aggregates we usually wish to predict, they must be used for control purposes and as a final check on prediction formulas *derived from less aggregative data*"(*ibid.*, pp.17-8, my emphasis). While macroeconomic phenomena are observable through statistical aggregates (consumption, gross domestic product, etc.) the Committee insists on the fact that the explanation of the phenomena and their prediction deserves to be made through a study of microeconomic data and models designed on the scale of the individual. With thousands of observations available, it is possible to compare and decide between different theories; *i.e*. test them.

Katona is very critical toward the *Smithies Committee*'s position. For Katona, the phenomena measured at the aggregate level are different from the phenomena measured at the individual level: the whole is different from the sum of its parts. Thus, the phenomenon that Katona seeks to measure is observable only through attitude aggregates (see part III), it is a holistic phenomenon. The negative position of Katona is thus clearly understandable because the Smithies Committee takes away the legitimacy of aggregate facts, and thus closes the door to the empirical test of Katona's theory. In addition, the conclusions of the report tend to valorize buying intention data against other attitudes, and Katona does not agree with that.

It should be noted that in parallel with the above findings, the report highlights a paradox of composition: the attitudes at the individual level are unstable, while at the aggregate level they are stable:

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together (correlated) and therefore it is difficult to identify which variable explains what other variable, (iii) Aggregate variables such as aggregate consumption "is not only affected by income but also affects the generation of income"(reverse causality).

[30] "Even though many of the sources of variations in individual behavior may not be relevant for a prediction of aggregate behavior, the gain in useful information is still very great." (Smithies *et al.*, 1955, p.16)

> "In the aggregate, the consumer attitudinal data are quite stable. This is true of purchase intentions and of attitudes toward the personal and general economic situation (including expectations about the future). This aggregate stability masks the fact, however, that many individuals reveal quite different attitudes from one survey to another. This kind of instability - that is, at the individual level - can be ascertained, of course, only by reinterviews. *But the fact of its existence raises questions about the significance of the stability of the attitudinal data in the aggregate*." (Smithies, 1955, p.35, my emphasis)

The stability of attitudes at the aggregate level means that their variation are limited and are not erratic. Conversely, individual attitudes fluctuate greatly. The Committee will not develop the implications of this paradox. Nevertheless, the report emphasizes that the stability of attitudes at the aggregate level explains the fact that they have better predictive performances than attitudes at the individual level. This paradox, as we shall see, is fundamental to understand Katona's position in the controversy presented below.

This controversy begins with Katona's critical comments on the Smithies report. He argues that the evidence at the individual level is neither necessary nor sufficient to test and demonstrate the predictive value of attitudes aggregated level. The position of the *Smithies Committee* and Tobin (supported by the Okun's paper of 1957), on the contrary, is that the evidence at the individual level is necessary and sufficient to demonstrate that attitudes have an effect on the aggregate consumption of households.

Two issues are addressed during the controversy: (i) What is the nature of psychological phenomena measured by attitudes? Is it an individual phenomena, or is it a social phenomena? This first issue is theoretical and ontological. (ii) What is the nature of the measures available through the SCF? Can we use the attitude data to construct aggregates? This question is about the nature of empirical facts, and about the possibilities given by the SCF to construct proofs. These double aspect of the issue, both empirical and theoretical, will affect the interpretation of data. For example, if the data is understood as a conscious and voluntary emanations of individuals, then the tests at the individual level will be favored. But in the same way, these theoretical and ontological positions are affected in return by the results issued from the data: the fact that purchase intentions for example works better at the individual level will encourage the theoretical development of purchase intentions. A third issue, the question: what do we measure (iii), make the interface between (i) and (ii).

To simplify the presentation of the controversy, which involves many arguments between the actors, the article summarizes Tobin's position and Katona's one and presents each of them into two separate parts. Three main items are used. Katona's response to the Smithies report in 1957, the article by Okun in the same year and Tobin's paper in 1959 (which is accompanied by Katona's response, and the final word by Tobin). These articles

constitute the heart of the controversy, although a set of various contributions, mainly empirical, are written during this period[31]. The results of these works are generally included in the arguments of the two main actors: Katona and Tobin. The second part deals with the position of Tobin and the third part is devoted to Katona's arguments.

II) From an issue on methodology to an ontological stand on the individual: Tobin and the micro-foundations of macroeconomics.

> "A cross-section test involves a microscopic study of the correlation in a sample of households between attitudes and intentions, on the one hand, and economic action, on the other. The relevance of such a test to the general question of the predictive value of consumer attitudes and intentions *seems to me self-evident*. Indeed I do not see how the predictive value of these data *can be adequately appraised without confronting the attitudes and intentions of individual households with the record of their subsequent behavior*." (Tobin, 1959, p.1, my emphasis)

The methodological individualism suggested by the *Smithies Committee*, outlining that, for some methodological reasons (like taking advantages of cross-sectional data), studies on the relationship between attitude and behavior would be improved if done on the individual level, will congeal into an ontological stand when it comes to Tobin. He sees individual attitudes collected through surveys as determinants of individual decisions to consume or to save. In other words, the involved psychology phenomena are at the individual level. Thus aggregating attitudes makes only sense when it reflects the sum of individual entities:

> "Katona criticizes the concern of the Smithies Committee, of which I was a member, for insisting on confirmation of predictive value at the level of the individual household. It is a travesty of the Smithies Committee report to attribute to it "the assumption… that the individual 'fulfillment' rate alone matters". Obviously what matters is the aggregate prediction. The question at issue is whether *one can have confidence in aggregate predictions based on the overall proportions of*

[31] We can cite: Lansing, J. B. et Whithey S., *Consumer anticipations: Their Use for Forecasting Consumer Behavior*, in Studies in Income and Wealth, Volume 17, NBER, 1955; Juster, T., *The Predictive Value Of Consumers Union Spending-Intentions Data*, in The Quality and Economic Significance of Anticipations Data, NBER, 1960 ; Juster, T., *Prediction and Consumer Buying Intentions*, The American Economic Review, Vol. 50, No. 2, 1960; Mueller, E., *Consumer Attitudes: Their influence and Forecasting Value*, in The Quality and Economic Significance of Anticipations Data, NBER, 1960; ainsi que les papiers de Klein cités en page 7 du présent article. Pour une revue assez détaillée de ces articles voir McNeil, J., *Federal Programs to Measure Consumer Purchase Expectations, 1946-1973: A Post-Mortem*, The Journal of Consumer Research, Vo.1, No. 3, 1974.

> *favorable attitudes in a sample, if these attitudes turn out to bear no relation to the behavior of the individuals who expressed them*." (Tobin, 1959, p.1, my emphasis)

In a milder version, the Smithies report already acknowledged such a claim:

> "... it would surely be very difficult to construct a plausible model of human behavior, even allowing for much purely random and idiosyncratic differences among individuals, *on which attitudes, could influence subsequent behavior of large groups without influencing the behavior of those who were observed to hold them*." (Smithies, p. 61, my emphasis)

The Committee also added the observation that attitudes are "observed" at the individual level. Thus, it is the methodological individualism implied by the methodology of surveys that presuppose (in the view of the *Smithies Committee* and Tobin) that attitudes are ontologically individual. To support this view, Tobin will try to demonstrate that no effect of aggregate attitudes could happen without an effect at the individual level. He finds a hunch in Okun 1957's article[32] :

> "To what extent do findings from cross-section data provide evidence regarding the aggregative predictive value of anticipations data? This is a crucial issue and it deserves consideration. Perhaps, *some formal analysis* can assist in clarifying the matter." (Okun, 1957, p. 29, my emphasis)

Let's summarize Okun's argument here. Knowing that at the start of a year *t*, a sample is surveyed on its intentions of buying a durable good. Part (p_t) of the population plans to do it, part ($1 - p_t$) doesn't plan to. At the end of the same year are evaluated both how many people honored their intention (designated by r_t), and how many bought a good although they didn't intend to (s_t). With a global buying rate of (x_t) for the entire group, the following relationship could be deduced:

$$x_t = r_t p_t + s_t(1 - p_t)$$

Okun develops his example and concludes:

> "In the special case where *r* and *s* are both independent of *p*, the answer is an unqualified "yes": the condition that intentions data have predictive value on a cross-section basis [...] is *both necessary and sufficient* to insure that they have predictive value in the aggregate." (Okun, 1957, p.30, my emphasis)

The "special" case underlined by Okun hypothesizes that *r* and *s* are independent of *p*, therefore inducing that there is no impact of the distribution of attitudes of the group (*p*) on its individual behaviors (*r* and *s*). In other words, an individual doesn't get affected by other individuals, which are referred here by the distribution of attitudes in society. Therefore, the intentions mean (at an aggregate scale) could lead to a prediction of a mean of buying behaviors only if each individual buying intentions are related to their own behavior. But if

[32] "I am indebted to my colleague Arthur Okun for formalizing the argument that lay behind the position of the Smithies Committee" (Tobin, 1959, p.1). The article, while describing the quarrel as a quarrel between Katona and Tobin, develops also Arthur Okun's argumentation. Although Okun has given a solid argument, Tobin came to be the one who spearheaded the discussion on a wider.

the hypothesis of the independency of *r* and *s* from *p* is removed, according to Okun:

> "If the intentions data have no predictive value at the household level, they may still conceivably have predictive value in the aggregate. The requirements are that either the probability of fulfillment by intenders or the probability of purchase by non-intenders varies directly with the volume of plans to buy in the whole economy. In such a situation, intentions to buy stimulate purchasing, but they are no more likely to influence those who express the intentions than those who initially expect not to buy. *Expectations are somehow symptomatic of the atmosphere*, without supplying any evidence concerning the individuals who express the particular expectations." (Okun, 1957, p.31, my emphasis)

Okun admits that if there is a dependency between individuals behaviors and the volume of plan in the whole economy (or the volume of favorable attitudes), the composition paradox between results at the individual and aggregate levels is legitimate. Paradoxically, Okun analytically yields on the case he tries to avoid: The connection in cross-section data between individual attitudes and individual behavior is not "necessary and sufficient" anymore to a connection between these attitudes and aggregate behaviors. One can question the value of Okun's formal example to solve the quandary. Nonetheless, it greatly clarifies the positions of Katona and Tobin. The "special case" assuming the independency of *r*, *s* and *p*, is the formal assumption of an ontological individualism. It is Tobin's view. On the contrary, the "general case", in which individual attitudes are potentially linked to the whole society, is Katona's view.

Nevertheless, Okun does not consider the latter case as a real life situation. To support his claim, he focuses on an instance of influenza pandemic which broke out in Asia in 1956. If at that time households were surveyed on the eventuality to get contaminated, the majority would answer the unlikelihood of such occurrence. By 1957, with the pandemic spreading and a more intensified media coverage, a bigger portion of surveyed households would have responded by the affirmative to the same question. Their feeling was evidently not associated with their probability of contracting the disease but with the dread of catching it.

> "The population can sense the presence of flu viruses in the atmosphere and still be totally unable to predict who will be stricken." (Okun, 1957, p.32)

Surprisingly, Okun skipped his example to the following conclusion:

> "It seems *highly unlikely* that such conditions could ever apply to *voluntary* economic behavior." (*ibid.*, my emphasis)

Tobin will claim it on his own[33]. Along his say, Okun holds an individualistic posture and neglects other factors such as the peer factor and the trend factor, which are phenomena with the same properties as the asian flu. Therefore Okun's assertion stating the existence of a

[33] "Although this special kind of model would make it possible to regard a reinterview test of the difference between r_t and s_t as irrelevant to the predictive value of over-all proportions*, it is a highly artificial and implausible construct for the voluntary economic decisions of households*." (Tobin, 1959, p.2, my emphasis)

propagation of factors such as the household confidence, concept championed by Katona, from person to person and that would shape their behavior, should be taken as "highly unlikely". This claim echoes the adjective in the aforementioned quote, that the economic behavior is "voluntary." Eventually, what Okun and Tobin object to Katona is an ontology of the individual behavior, which is that individuals are conscious of their actions and of what affects them.

The argument is well founded enough for Tobin in 1959.

Besides Okun's argument Tobin also emphasize a methodological one, already mentioned in the Smithies report, yet reaffirmed by Tobin himself:

> "Second, the necessity of testing at the individual level the predictive value of attitudes and intentions follows inexorably *from the inadequacy of any other kind of test*. Aggregate statistics from successive surveys form a time series that can be compared with aggregate time series of consumer purchases or saving components. In the paper cited above, Okun gives a thorough review of the evidence provided by comparing these aggregates. He finds the evidence inconclusive, and it could scarcely be otherwise. Only 11-13 observations are available. Since these are roughly quarterly observations, the notorious serial persistence of economic time series makes it doubtful that there are as many as 11-13 independent observations." (Tobin, 1959, p.2, my emphasis)

Tobin dismiss all time-series proofs as, with the narrow amount of observations, it is not possible to interpret econometric estimations. He infers:

> "If Katona believes he has observed that changes in an attitudinal index lead changes in expenditures on durable goods, he has not based this belief on any *rigorous statistical* test." (Tobin, 1959, p.3, my emphasis)

The use of this second argument is rather odd. First, one can wonder why Tobin wields it after theoretically withdrawing the potential impact of aggregated attitudes, if they have no repercussion on the individual scale. At the same time, Tobin somehow asserts that, has such a factor might exist, *i.e.* a potential impact of the society on individuals composing it, no statistic demonstration might be elaborated to prove it—such a proof would be technically illegitimate to him, hindered by the small number of available observations and by the affirmation that in macroeconomy, there could never be such "independent observations." The couple of Tobin's arguments beforehand shuts to any reasoning Katona might have on his quest for macropsychological factors.

Next, Tobin evaluates Katona's and Mueller's results through a composite index of attitudes, the precursor of the well-known confidence indexes[34]. Weighting in the pros and cons made

[34] As mentioned earlier (cf. footnote n°27, p.9), aggregate tests require aggregates to be built from individual surveys. The *Smithies Committee* restricts itself to the calculation of ratios of favorable attitudes to unfavorable attitudes. Katona has been experimenting the construction of various indexes since 1951 with Eva Mueller. These indexes are generated from multiple attitudes (from 4 to 6 including both attitudes and intentions.) Each positive attitude increments the index by one, each negative attitude decrements it by one, both done without any weighting between attitudes of different questions composing

by Eva Mueller and Arthur Okun, he concludes:

> "The main lesson to be drawn is that the observations are too few and too ambiguous to permit conclusive tests." (Tobin, 1959, pp.5-6)

Arthur Okun's and James Tobin's postures might then look as intellectually dishonest. Their statement is strident:

> "Prof. Katona is undoubtedly still on firm ground in arguing that 'very little has yet been proved' concerning the predictive value of intentions, on the one hand, and of other expectations and attitudes, on the other. [...] at this point it appears *that the burden of proof* falls on those who would contend that the predictive value of intentions data can be materially augmented by information on attitudes and other expectations. If, in the next Survey of Consumer Finances, the latter group of variables and the intentions data offer conflicting omens concerning the prospects for consumer spending, the empirical record to date would oblige the forecaster to follow the lead of the intentions data." (Okun, 1957, pp.40-1, my emphasis)

On the same tone, following Katona's article in 1959, Tobin shots the following words at him—which also are the last words made on their quarrel:

> "But let them [Katona et Mueller] be modest in claims of success in this quest until they are in a position, not just to explain away negative results, but to cite favorable results of *rigorous statistical tests*." (Tobin, 1959, p.319, my emphasis)

Tobin and Okun request Katona to produce rigorous statistical proofs of his theory. This posture is somehow unfair as they have in the meantime vetoed any rigorous proofs of a holistic theory. Broadly speaking, to Tobin, Katona's ingenuity mostly lays in his ability to pioneer the collection of attitudinal data[35].

III) Katona and the macro-foundation of microeconomics : Toward a Macropsychology

> "Just as proponents of a new theoretical approach speak in enthusiastic terms, so those who reject their approach often appear ready to condemn it on inadequate evidence." (Katona, response to Tobin, 1959, p.317)

Katona will give his response partly during the controversy at the end of the 1950s (Katona, 1957, 1958, 1959) and partly after the controversy during the 1970s (Katona 1975,

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the index. The index is then normalized to yield a median value out of 100 on its long run. Confidence indexes that have been used in more than fifty countries until now are derived from the pioneers experiments in the fifthies.

[35] "Thanks to the experience they are accumulating, *we* can investigate the questions which attitudes are the most important ones to investigate in periodic surveys and what is the best way to use these data in combination with other economic information" (Tobin, 1959, p.11, my emphasis). One could see here that the "we" refers to Tobin's approach of the investigation and not to Katona's one.

1979). Although Katona's defense is already convincing in 1957 his theoretical position will gain in consistency thereafter. It should be emphasized, for example, that his 1958 article is fundamental for the macropsychological approach developed in his 1979 article: *Toward A Macropsychology*. However, Katona does not make the direct link between this article and the controversy. Thus the controversy reveals a Katona taken aback by the criticism of Tobin and of the *Smithies Committee*[36]. This part is constructed in the light of Katona's late writings, even if the main arguments are taken from his writings published during the controversy.

Katona must answer to the criticism made by Okun (saying that it is "highly unlikely" that there is a dependency between individual attitudes and the attitudes of the whole society) on the one hand, and on the other hand he must answer to the methodological claims arguing that cross section data are more reliable than time series. To give a solution to those problems, Katona develops the following question : What do surveys actually measure?

Facing the composition paradox stated above (*cf.* p.12 : the stability of attitudes is observable at the macroeconomic but not at the microeconomic level), two positions emerge. The first position, that of the *Smithies Committee* and that of Tobin after it, leave aside the question and try to find a stable microeconomic foundation. The second position, that of Katona, will be quite different. Indeed, both the instability of individual attitudes and the stability of global attitudes are psychologically justified:

> "To be sure, consumer attitudes might undergo a change shortly after a survey has been concluded. Yet psychological theory postulates, and past experience confirms, that the attitudes and expectations of broad groups of people hardly ever change abruptly, except under the impact of major events." (Katona, 1957, p.41)

The instability of individual attitudes is justified for two reasons. The first one is theoretical and argues that the instability of attitudes is a natural characteristic of individuals: their attitudes depend on their personal characteristics (social background, feeling of belonging to particular groups, the natural optimism of some people) and their "personal experience" (their mood, a raise or conversely a decline of their salary, the contraction of a disease etc ...) (Katona, 1958, p.1). The second reason is methodological: the instability of individual attitudes stems from the methodology of surveys which generates "misclassifications"[37]:

[36] It should be recalled that Katona' s team had little time to analyze the SCF data and produce research based on it: "None of the contracts which financed these projects [*Survey on Consumer Finance*] called for the integration of findings from different projects *and the theoretical interpretation of these findings*. In view of the fact that the writing of such a book [*Psychological analysis of Economic Behavior*] has to be done almost completely outside of "office hours" and in addition to full-time contract commitments, it is not surprising that Katona's is the only book-length theoretical publication from the Institute at the present time." (*Survey of the behavioral sciences : report of the faculty committee and report of the visiting committee*, p.95)

[37] See also: Katona, 1960, pp. 254; Maynes, 1967, p. 121.

> "In a narrow sense this term [missclassifications] includes clerical errors, interviewing errors, misunderstanding by respondents, and reporting errors. In a broader sense one may include in the same category effects of the mood of the respondent, and changes resulting from guessing or expressing ad hoc opinions when a respondent does not know the answer but is induced to reply." (Katona, 1958, p.1)

These two arguments explain why an individual test of the relation between attitudes and behaviors is difficult, or impossible to obtain. Indeed, an individual attitude measured at a period *t* is certainly disconnected from the behavior measured at a time *t + six months*:

> « We follow widely accepted lines of psychological thinking about group effects when we contradict the assumption of the report that the individual "fulfillment" rate alone matters. Of course, some effects should be, and are, traceable on the individual level. *But individual behavior is influenced by many specific factors peculiar to the individual's situation, in addition to his predispositions. These specific factors may lower the individual fulfillment rate*. » (Katona, 1957, p. 42, my emphasis)

This theoretical and methodological claim (*i.e.* individual attitudes are unstable) establishes a logical restriction to the test of the attitude-behavior relationship at the individual level. It should be noted that the position of the *Smithies Committee* and Tobin ignores this problem. The stability of aggregate attitudes, and its relative correlation with certain economic developments is a sign for them that there is stability at the individual level between attitude and behaviors.

A possible way defended by Tobin and the *Smithies Committee* is that the observed individual instability could be reduced by an improvement of the survey methodology. Part of the criticism of this approach is already contained in the previous answer, since any method will always face the problem of natural instability of individual attitudes. For Katona, "A model which deals with the relation between individual attitudes and situations on the one hand and individual behavior on the other would be more cumbersome and more difficult to handle than an aggregative model" (Katona, 1957, p.42). Furthermore, collecting the data needed to support such a model would give rise to a set of challenges. It would require a representative panel of the American people which would be not only expensive, but also subject to biases due to panel mortality (Katona, 1957). Households move, split, etc. and the sample would be less and less representative when time flies (Klein and Lansing, 1951, p.111). Furthermore, reinterviews involve making two surveys for the same sample, which is twice as expensive.

A significant shift that accompanies the conversion from the problem of attitudes-behavior relation to the problem of the stability of attitudes is that Katona endorses the concept of attitude itself as a dynamic concept. Not dynamic in the sense that the causality runs from attitudes to behaviors, but in the sense that it is a change of attitudes that leads to a change in behavior. Thus, observing the dynamic of attitude change implies a dynamic measure of attitudes, and not a punctual one like in cross sections data, which is obtainable through time

series :

> "For at least one important purpose reinterviews are not needed and are less satisfactory than consecutive surveys with different representative samples. This purpose is the establishment of trends of economic as well as psychological variables over time." (Katona, 1957, p. 43)

At this point, Katona has to explain why aggregate measures can be a source of valuable information: that is to say, on the one hand (i) why attitudes are stable at the aggregate level, and also (ii) how, when and why changes in aggregate attitudes make sense and explain the behavior of groups of individuals.

The answer to item (i) is evident for Katona: Instability of attitudes due to personal experiences and misclassifications "may be unimportant in the aggregate because they may cancel out." (Katona, 1958, pp. 1-2). This argument will be developed in his 1979 article where Katona says that this is an application of the "law of large numbers". The different individual traits are randomly distributed in the population and it is the same for other personal experiences:

> "That law has occasionally been expressed in a way that may appear to be a suitable formulation of the experience with the Index of Consumer Sentiment, namely, that what individuals will do is uncertain, but what hundreds or thousands of individuals will do is not equally uncertain." (Katona, 1979, p.1)

Erratic movements observed at the individual level therefore cancel out and make visible trends at the aggregated level. By extension, and this is the response to item (ii), some macropsychological influences can have a significant effect on all households. These influences may appear, for example, after the publication of important news (a large variation of prices, a government intervention, new geopolitical issues, etc.). Katona's objective is then to explain this macropsychological change. That means looking into the reasons given by survey respondents for their attitudes. This is why open-ended questions like "Why do you say so?" are systematically asked in surveys (Katona, 1946, 1951, 1975, 1980)).

Macropsychological phenomena have even more chance to have an impact because according to Katona, "such attitudes and expectations as waves of optimism or pessimism spread like a contagious disease rather than in a random manner." (Katona, 1979, p. 120):

> "Our perceptions are selective. Why is it that at certain times the same aspects of information become influential? Here we note the effect of groups on the individual. People belonging to the same face to-face group or having similar interests (*e.g.*, the occupational groups) have the same kind of apperceptive mass. They are predisposed to select the same information and to understand the information they receive in a similar manner. There also exists pressure toward uniformity both in selecting the information and in understanding it. Finally, there is mutual reinforcement of information among group members. The aspects of information people tend to discuss in their group are those with which they most strongly agree or disagree, and these conversations tend to result in uniform attitudes. No doubt many individual differences remain even among members of coherent groups." (Katona, 1958, p. 35)

Macropsychological developments are explained by the concept of "social learning"

("macrolearning" in his 1979 article). This concept designates a phenomenon related to Katona's theory of learning[38]. We see here that Katona clearly adopts an opposite view compared to that of Okun. For him the attitudes and behavior of households are constantly influenced by the attitudes of other households. The individual opinions collected through surveys therefore are a way to measure the footprint of the social atmosphere on individual. This footprint is made visible by the aggregation of attitudes. Tobin does not seem to be willing to understand the theory and rejects it without giving much consideration to it (and this seems to be also true for Okun). At some points, Tobin is ironical with Katona's theory:

> "Katona holds that it is the *Gestalt or cluster of attitudes*, rather than attitudes taken singly, that matters." (Tobin, 1959, p.5, my emphasis)

Tobin shows irony, because he suggests an equal meaning between the body of *Gestalt* theory and the indexes Katona tried to build in order to capture the effect explained above[39].

For Katona, reinterviews are explicitly needed to support the study of the causes behind changes in aggregate attitudes. Katona produces such a study in 1958 in his article *Attitude Change: Instability of Response and Acquisition of Experience*. Its purpose is to distinguish macropsychological changes induced by a strong volatility in individual attitudes (many individuals switching from positive to negative attitudes and vice versa), and changes induced by a high proportion of the sample switching in the same direction (Katona, 1958, 1979). In his 1979 article, for example, Katona defines uncertainty as a macropsychological state characterized by a high volatility of individual attitudes in which no precise trend can be identified.

To conclude this part, Katona gives convincing arguments in response to the criticisms of Tobin and Arthur Okun. He challenges on the one hand the possibility of ever finding an attitude-behavior relation at the individual level. On the other hand he defends and explains the stability of attitudes at the macroeconomic level and the origins of its potential influence on aggregate behavior. However there still is a missing piece in Katona's argument: How can

[38] The major contribution by Katona to the *Gestalt* is his book *Organizing and memorizing: Studies in the Psychology of Learning and Teaching* (1940), and is a study centered on the idea that memorizing is better achieved through an understanding of problems and their solutions rather than trying to memorize by repetition. See Edwards (2011) for a short presentation of this study. For Katona, individuals have a frame of reference which is influenced by others, and that shape their understandings of economic situations. At some period, a social switch in people's frame of reference can happen, so that at the aggregate level, behaviors will evolve.

[39] The original quote of Katona that Tobin is refering to is: "The basic tenet of Gestalt psychology in which our studies originated is, however, that a part or item may change its meaning and function according to the whole to which it belongs. Thus it is not at all surprising that expecting prices to rise was at certain times a factor promoting and at other times a factor retarding consumer purchases. Nor is it unexpected to find that the feeling of being "better off" influences spending differently when it is associated with an expectation of future financial improvement than when it is associated with an expectation of future financial setbacks. Instead of testing the predictive value of each attitude separately, the relation of clusters of attitudes to behavior should be studied." (Katona,1957, p.42)

the influence of attitudes on the aggregate behavior be proved? In other terms, how can we empirically distinguish between a macroeconomic effect along a traditional economic explanation (consumers consumed less because the prices went up) and a macropsychological effect along the theory of Katona (consumers consumed less because they saw the rising prices as a negative sign for the future developments of the economy)?

Conclusion: Macropsychology and prevision: a complex recipe

The controversy had a major impact on the development of attitude surveys after 1959. As stated by McNeil (1974), the *Federal Reserve* financed new programs such as the *Quarterly Survey of Consumer Buying Intentions* (QSI), first published in January 1959. This Survey focused on buying intentions data (following the recommendations of Tobin and the *Smithies Committee*). The results of the QSI were mixed, macroeconomic previsions obtained from the QSI being not accurate. The *Federal Reserve* decided to discontinue the QSI in 1967 and developed the *Survey of Consumer Buying Expectations* (CBE). The novelty of the CBE consisted in the use of a subjective probability methodology. Both the QSI and the CBE were thus developed along the idea that the purpose of the surveys was to measure more precisely what was the true individual probability of buying (along the line of the "voluntary" view of economic behavior), and did not take into account Katona's theoretical recommendations. The CBE was discontinued in April 1973 (McNeil, 1974). From this period and until now, the SRC maintained an attitude survey along Katona's line, the *Survey of Consumers*, with private funding. The history is paradoxical. On the one hand the historical developments after the controversy shows that Tobin and the *Smithies Committee* influenced the *Federal Reserve* funding. On the other, all the subsequent programs were discontinued, and nowadays, the remaining surveys all around the world are closer to Katona's view[40]. To conclude we can briefly summarize what Katona's research agenda became from the sixties to 1981, the date of his death.

Macropsychology seeks to theorize the relation between aggregates of attitudes and mass behavior. The first difficulty of this program is (i) to construct an object that can account for these attitudes at the aggregate level, namely an index of attitudes. The construction of

[40] On the dissemination of confidence indicators, see Curtin R. T., *Consumer Sentiment Surveys : Worldwide Review and Assessment*, Journal of Business Cycle Measurement and Analysis, OCDE, 2007, p. 16.

such an object is inherently complex. What attitudes do we incorporate in this index (knowing that many attitudes are correlated)? How to operate the aggregation between attitudes (should we weigh the attitudes and assign a stronger influence to some of them)? Katona doesn't have accurate answers to these questions and usually recalls the experimental nature of the various indexes he constructed (Katona: 1959, 1960, 1975).

The index being constructed, it is necessary to explain the variation of the latter with the variation of aggregate macroeconomic variables (ii). The history of these attempts is thick. These correlations are necessarily expressed through time series data facing a number of methodological difficulties, the same difficulties that the one emphasized by the *Smithies Committe*e. Which period should be chosen to estimate the models ? Which macroeconomic variables should be included in the model?

Finally, the correlation between the index of attitudes and the macroeconomic variables need to be established as a causal relationship going from attitudes to macroeconomic variables (aggregate consumption for example) (iii). However, it is often possible to show that the index was explained by economic variables. In that case, attitudinal data is a mirror of economic developments and doesn't provide significant information. The theory of the independent impact of psychological variables in this case is highly questionable.

These three difficulties have no direct solution and will always face the Duhem-Quine problem. How to explain a positive empirical result between an index and aggregate consumption, for example? Did the empirical model control for all the macroeconomic variables? Can a result obtained through these estimations be generalized to other periods? What would have happened if we had changed the questions integrated in the indicator? All these issues show that it is never possible to test all the parameters involved in the question of the attitude-behavior relation. In other words, it does not seem possible to provide *rigorous statistical* evidences of this theory. Thus one can have doubts on the fact that it is possible to converge over time towards a satisfactory theory.

Okun is aware of this difficulty :

> "These considerations point to the variety of ways in which anticipations data may be employed in forecasting. Because of the multitude of possible uses, it is exceedingly dangerous to render an overall judgment concerning the predictive value of any expectational variable. Any investigator rash enough to declare that a series has no value in forecasting is stating merely that he has discovered no fruitful use. He may find himself embarrassed in short order by the research of a more ingenious or more fortunate economist. On the other hand, a favorable verdict concerning any series may be upset by a demonstration that equally good results can be obtained without reliance on that series. The appraisal of the predictive value of various types of data is inherently a risky business. Any evaluation should be advanced, and interpreted, as being tentative and as resting on a *pragmatic foundation*." (Okun, 1957,

p.3)

This observation brings us back to the starting point in the 1950s. The will of the *Smithies Committee* to find rigorous statistical basis through individual tests does not seem convincing in the light of Katona's arguments. This is true from a theoretical point of view as well as from an empirical point of view. Pragmatically (to use Okun's words in the above quote), programs financed by the *Federal Reserve* in the wake of the controversy were unsuccessful in predicting macroeconomic developments in consumption and savings (Edwards, 2011; McNeil, 1974). The controversy shows us a strong opposing position, that of Katona, on the question of the microfoundation[41] of macroeconomics. Katona defends that all macroeconomic phenomena cannot be simply reduced to individual phenomena, an idea which is swimming against the current of the period. However, the inherent difficulty to construct statistical proofs of such a theory, proofs meeting the standards of the scientific community, impeded its theoretical development and legitimacy.

[41] We refer here to the *aggregation program*, one of the three programs of microfoundation defined by Hoover (2012).

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